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THE JOURNAL OF PHILOSOPHY

PHILOSOPHICAL ANALYSIS, TRANSLATION SCHEMAS, AND THE REGULARITY THEORY OF CAUSATION

THE problem of the adequacy or inadequacy of Hume's analysis of causation, the so-called "Regularity Theory," is one of those problems which any philosopher who wishes to say original things will enter upon with some diffidence. It has been discussed almost literally *ad nauseam*. Lately, it has become intimately tangled up with the problem of the contrary-to-fact conditional. But I hasten to announce that the problematic aspect of the regularity theory to be discussed in the following has nothing to do with the latter problem at all. A difficulty with the Humean analysis which has received insufficient attention in the heat of debate about the meaning of "necessary connection" is, just how the *classes* of "constantly conjoined" events, relevant to a given singular causal judgment, are to be selected. And I shall argue that the only "solution" of this difficulty consists in the recognition that the regularity theory is not an "analysis" in quite the same sense in which some other philosophical theories are "analyses."

Let us plunge at once *in medias res* and ask just how the causal judgment "the impact of the stone caused the window to break" should be translated into a statement about a regular sequence according to the regularity theory. It is easy to state one component of the *analysans:* "there occurred an impact of a stone against the window, and this event was immediately followed by the breaking of the window." But this is, of course, no complete analysis. If it were, then the fallacy "post hoc, ergo propter hoc" would be no fallacy at all. In condemning this mode of inference as fallacious we are just saying that "*c* immediately preceded *e*" (where the small letters are chosen to represent dated events, in contrast to repeatable kinds of events) does not entail "*c* caused *e*." For any effect, *e*, there is a vast multitude of mutually simultaneous immediate antecedents: right before the window broke, somebody in the room sneezed, somebody else looked out of the window, somebody else recited a Shakespeare poem, and it is *a priori* possible, as Hume clearly saw, that any of these antecedents caused the effect. What we need, then, is a criterion which effectively distinguishes the causal antecedent from the fortuitous antecedents, and it is

here that the reference to *regular sequence* comes in. But it is easier to describe, in this manner, the *kind* of statement we require for a complete analysis of the causal judgment, than to pick a specific instance of the kind. Is part of what we mean by our explanation of the breaking of the window the law "whenever a stone impinges upon a window, then the window breaks"? Surely not; for we know that this type of antecedent is sometimes not followed by the specified effect, and still we are convinced that, *in this particular instance, c* was the cause of *e*. This type of analysis breaks down particularly if the singular causal judgment has the subjunctive form "if *c* had not occurred, *e* would not have occurred": *c* is merely asserted to be a necessary condition for *e*, hence it is not regular *sequence*, but at best regular *precedence* that could be implied. But the analysis "an event of the kind of *e* is regularly preceded by an event of the kind of *c*" breaks down on account of the plurality of causes. The fainting of a woman may, in a given instance, be caused by her belief that her son just died, in the sense that she would not have fainted had she not held that belief; but obviously this kind of effect is not regularly preceded by such a belief, it can be produced by any number of purely physiological causes.

A Humean might now retort that obviously no synthetic, universal implication "whenever A, then B" can be known with certainty; that no matter how specifically A, the antecedent, be described, it always remains conceivable that in a given instance A fails to be followed by B. But this merely proves, he might argue, that on the Humean analysis any singular causal judgment falls short of certainty. This defense is weak, however, since it is not just *conceivable* that a counter-instance to a generalization like "whenever a stone impinges upon a window, then the window breaks" might be found, but counter-instances *have been* found; hence the generalization is known to be false (which is different from knowing it to be no more than probable!), yet the singular causal judgment is firmly believed to be true. The Humean may now come back and say that the generalization implicit in the singular causal judgment refers to a narrower class of antecedents than the class suggested by the description of the supposed cause (the class of impacts of stones against windows, in our example). If, for example, A were described as "impacts of stones with minimal momentum M against windows," it would be far more difficult to think of counter-instances to the generalization. Similarly for the example of the fainting woman: there are, indeed, known instances of women receiving such terrible news without fainting, but if we characterize the women generalized about as having the sensitivity

of the woman whom the bad news caused to faint, then we get a pretty reliable generalization after all.

But it can be shown that this line of argument is really suicidal for the regularity theory. The difficulty, here pointed up, is, to repeat, this: suppose a proposition, *p*, which is firmly believed is claimed to be analyzable into a conjunction, *q.r*. Then an obvious criterion of adequacy to be satisfied by the analysis is that the analysans, *q.r*, should be as credible as the analysandum, *p*. In particular, then, if *q* is known to be false, it cannot be a component of a correct analysis of *p*, the proposition which is firmly believed. Now, the method of defense of the regularity theory under discussion is to make the credibility of the generalization which is allegedly implicit in the singular causal judgment proportionate to the high credibility of the latter by *narrowing the generalization down:* for constant B, the chance of finding a counter-instance to "whenever A, then B" is proportional to the size of A. Clearly, then, the credibility of the generalization will reach a maximum if A is narrowed down to a unit-class comprising as its only member the causal antecedent under discussion! That under exactly similar circumstances an instance of A will always be followed by an instance of B is, we might say, vacuously certain since the proviso "exactly simliar circumstances" precludes there being *another* instance of A. If this argumentation looks like hair-splitting, the following consideration should secure it the respect it deserves. In self-defense against the charge that they obliterate the distinction between coincidence and natural necessity, the Humeans have pointed out, with some justification, that on the contrary the regularity theory provides the only intelligible analysis of this distinction that has ever been given: to say that B accompanies A, not just by coincidence, but by natural necessity, is just to say that the correlation is *invariable*. "If C is *regularly* followed by E, then C is the cause of E; if E only 'happens' to follow C now and then, the sequence is called a mere chance" (Schlick, "Causality in Everyday Life and in Recent Science"). And Reichenbach is equally unambiguous on this point: "Since repetition is all that distinguishes the causal law from a mere coincidence, the meaning of causal relation consists in the statement of an exceptionless repetition—it is unnecessary to assume that it means more" (*The Rise of Scientific Philosophy*, p. 158).

But suppose, now, that *c* is followed by *e* and the question is raised whether this is mere coincidence or a causal sequence. According to what Schlick and Reichenbach tell us, the question means whether *c* and *e* belong to *classes* of events C and E such that *every* member of C is followed by some member of E. We have seen that

if the abstracted classes are fairly comprehensive, one is likely to obtain a generalization which is known to be false and which therefore cannot be intended by the singular causal judgment. But what if the description determining C is so specific that C has very few members (and in the limiting case just one member)? The quoted definitions of "coincidence" imply that in this case the sequence is clearly causal! The point can tellingly be made in terms of Reichenbach's own illustration:

> It once happened that while the screen of a motion picture theater showed the blasting of lumber, a slight earthquake shook the theater. The spectators had a momentary feeling that the explosion on the screen caused the shaking of the theater. When we refuse to accept this interpretation, we refer to the fact that the observed coincidence was not repeatable. [*Ibid.*, p. 158.]

Just suppose that the described motion picture had been destroyed by a fire right after its first showing which was accompanied by the mentioned earthquake, and that no other motion picture showing a similar episode had ever been made or will ever be made. The class of events "the screen of a movie theater showing the blasting of lumber" is on that supposition a unit-class, and if C is thus specified, then the sequence C—E is bound to be exceptionless. If Reichenbach should reply that nevertheless the sequence *c*—*e* must be regarded as a mere coincidence since if we select as C a larger class, such as "the screen of a movie theater showing some kind of blasting," then C has members which are known not to have been followed by a member of E, he would play into the hands of the above attack on the regularity theory: it is always possible to abstract a sufficiently large class C from the individual event *c* such that the sequence C—E has exceptions. Thus this argument would prove that every sequence is a mere coincidence.[1]

The regularity theory, then, is vague, or fails to offer an effective criterion for distinguishing coincidence from causal connection, because "exceptionless regularity" has no clear meaning if the class C is not specified and we are given no definite rule for selecting C. The point is of sufficient importance to deserve further clarification

[1] In thus criticizing Reichenbach's simple (perhaps deliberately oversimplified) analysis of the distinction between chance and causal necessity, I have indeed presupposed that "if an instance of C occurs at t, then an instance of E occurs at $t + dt$" is synonymous with "there are no instances of C which are not followed by an instance of E." This is, of course, tantamount to the untenable assumption that the extensional concept of formal implication is adequate for the expression of causal regularities. But the assumption is justified by the context in which it is used, since Reichenbach's claim is that the chance-causation distinction is reducible to a mere difference of *degree of regularity* of sequence, without saying anything about the distinction between *actual* and *possible* exceptions to a sequence of events.

in terms of illustrations. In the context of a critique of the regularity theory, with particular reference to historical causation, Morris Cohen writes:

> Suppose it were established that between . . . and . . . every invasion of or rebellion in the Roman Empire was preceded by a disturbance in the trade routes between China and Rome. Clearly that would not be sufficient to prove a causal relation. For if previous or subsequent invasions or rebellions had no such antecedents, or if "disturbances" of these trade routes were not at other times followed by wars of this kind, the assertion that the one event was the cause of the other would be definitely disproved. [*The Meaning of Human History*, p. 103.]

Curiously, Cohen here *attacks* the regularity theory in terms of just the sort of argument which the advocates of the theory would use to show that a given sequence was a mere coincidence and not an instance of causation! But waiving this weakness of Cohen's argument, the moral to be derived from the quotation is this: we are told that the singular causal judgment "the rebellion which occurred in the Roman Empire at t was caused by the disturbance in the trade routes between China and Rome at $t\text{-}dt$" would be warranted only if *every* rebellion in the Roman Empire had been preceded by a disturbance in the trade routes between China and Rome. Suppose, now, that this restricted regularity were actually a historical fact. What should prevent us from demanding, for the proof of the singular causal judgment, evidence of a more extensive regularity, say, that every rebellion anywhere should be preceded by disturbances in the trade routes between that country and some country with which it normally trades? But the more extensive the demanded regularity, the smaller the chance of justifying the singular causal judgment; and the more restricted the regularity adduced as evidence, the greater the chance that someone will complain that the evidence is insufficient to prove that the sequence is more than a coincidence. So, in the absence of a definite rule for selecting the classes with reference to which regularity is defined, the distinction between chance and causation remains arbitrary.

At this point the Humean may turn around and accuse us of having missed the whole point of the regularity theory. "You expect me to produce a rule which determines for any given singular causal judgment a *definite* law implicitly referred to by the judgment. But the regularity theory has never been claimed to be such a rule. What the regularity theory asserts is simply that *some* law is part of what is meant by any singular causal judgment[2]; which assertion

[2] A variant of this formulation is the formulation in *Elements of Analytic Philosophy* (where I tried to sell the regularity theory as best I could): "Every meaningful or verifiable causal statement which refers to *singular* events or

entails Hume's central thesis that a single observation is never sufficient evidence for any causal statement." Assuming that the regularity "theory" is intended as a rule for translating statements of the form "*c* caused *e*" into synonymous statements not containing the word "cause," just the way the frequency "theory" of probability is a proposed rule for translating probability-statements into synonymous statements about frequencies and not containing the word "probability" at all, we may put this statement of the theory into the form of the following contextual definition:

c caused $e =_{df}$ *c* immediately preceded *e*, and there is a class C and a class E such that *c* belongs to C and *e* belongs to E, and such that every member of C is followed by some member of E.

It takes, however, little acumen to see that this definition is wholly inadequate owing to the complete freedom it grants in the selection of classes C and E. As remarked already, if C is suitably narrowed, the definiens, and therewith the definiendum, may be made trivially true no matter how causally unrelated *c* and *e* may in fact be. And it can be shown that events which are pre-analytically known to be causally unrelated turn, on the above definition, into causally related events even if larger classes are selected. Russell's example of the hooter causing the workmen to leave the factory illustrates this: just substitute for "E" workmen leaving a factory at *t*, and for "C" a hooter sounding at *t-dt*; the individual event "workmen leaving the Manchester factory at t_o" is a member of the class E, and the individual event "the hooter sounding in the London factory at t_o-*dt*" is a member of the class C! It has been argued, indeed, that the reason why Russell's example of regular sequence is not a causal sequence is the absence of spatial contiguity between cause and effect, and that therefore the regularity theory easily survives the criticism if we follow Hume and incorporate the requirement of spatio-temporal contiguity into it. But there is a major objection to this line of defense. If spatio-temporal contiguity were part of the *meaning* of "causal sequence," then the conception of action at a spatial distance (such as gravitation was conceived to be before Einstein and such as telepathy is conceived to be by some) as well as the conception of action at a temporal distance (what Russell has called "mnemic causation," i.e., direct causation of a memory by an experience in the distant past)[3] would

facts is a substitution-instance of some law that refers to entire *classes* of particulars" (p. 216).

[3] For a detailed discussion of the problem of mnemic causation, see Russell, *Analysis of Mind*, Ch. 4, and especially Broad, *The Mind and its Place in Nature*, Ch. 10.

be *self-contradictory*; which consequence is highly doubtful. Furthermore, it would follow that causal efficacy could not be significantly attributed to mental events which cannot be significantly located in space: what could be meant by spatial contiguity of a volition and a muscular contraction! Thus epiphenomenalism, that unfortunate progeny of the billiard-ball model of causation which dominated the age of "mechanism," would be the inevitable concomitant of such a requirement.

The sort of criticisms of the regularity theory which I have adduced are no doubt behind the defeatist stand of those philosophers who hold that the concept of causal sequence involves an unanalyzable concept of necessary connection and that regularity of sequence, while it may be a more or less reliable sign of causation, forms no part of the concept of causation at all (as a classical member of this school, Broad comes to one's mind). I am going to suggest, however, that the root of the trouble is the confusion of a *rule* of translation with a *schema* of translation. By following a rule of translation one can translate any given instance of a given statement-form into a synonymous statement from which the term in question is eliminated. A schema of translation indicates the *form* of statement which a statement of a given analyzable form is translatable into, but provides no means for actually carrying the translation out for specific statements of that form. What are called "theories" of analytic philosophy are sometimes rules and sometimes schemas of translation, and this ambiguity of the term "philosophical *theory*" may be responsible for a good deal of confusion.

A clear case of a "theory" in the first sense is the frequency theory of probability. Whether or not it be adequate, it is a rule for translating statements of the form "the probability that a member of class K has property P is r" in such a way that the term "probability" drops out. Further examples of "theories" of this sort are: the logistic theory of number (rules, in the form of contextual definitions, for translating arithmetic statements into statements containing only logical constants); the causal (or "dispositional") theory of meaning which claims that a statement like "'dog' means dog" is translatable into a statement about a dispositional property of the word "dog"; the verifiability theory of meaning which says that statements of the form "S is meaningful" or "S means the same as S'" are translatable into statements containing such terms as "verifiable" and "verifiable consequences"; the linguistic theory of logical necessity, according to which "S is logically necessary," is translatable into something like "S holds by virtue of linguistic rules alone."

A good example of a schema of translation, on the other hand, is the phenomenalist theory of material substance, which was sketched by Berkeley and has been emphatically advocated in the "formal mode of speech" by Russell (following whose metaphor, it may be called the "bundle theory" of substance). If the dictum "a thing is a bundle of compresent qualities" were meant as a rule of translation then it would reduce simply to the claim that "*x* is a thing" is synonymous with "*x* is a bundle of compresent qualities." But in this form the theory would have hardly any application to ordinary language since, while ordinary language contains plenty of statements of the form "proper name (or definite description)—copula —concrete noun"—such statements as "the piece of fruit before me is an apple," "that animal is a bear"—it does not contain statements like "the rose is a thing" (pseudo-object-statements, as they used to be called). What Russell rather intended by that dictum is that the true meaning of subject-predicate statements would be best expressed in a language devoid of proper names and containing plenty of quality-names instead; there they would be replaced by statements asserting that a given quality is compresent with a given class of qualities. But suppose we granted the theoretical possibility of translating the subject-predicate statements of ordinary language into the ideal language disinfected of Aristotelian metaphysics, would the bundle-theory provide us with rules for carrying the translations out? Would we know how to translate such statements as "this is a tree" and "this tree is tall" into the ideal language? But surely "tallness is compresent with the class of qualities connoted by 'tree' " would not do: if the name "tree" must disappear, then its (meta-lingustic) name " 'tree' " must disappear too. We are told by Russell that "most subject-predicate propositions, such as 'Socrates is snub-nosed,' assert that a certain quality, named by the predicate, is one of a bundle of qualities named by the subject—this bundle being a unity in virtue of compresence and causal relations" (*Human Knowledge*, p. 84). This may well be true. But it only tells us what form the analysans, if it could be produced, would have, without telling us how we could produce a *specific* analysans for a specific analysandum such as "Socrates is snub-nosed." In order to do the latter, we would have to know a *defining property* of the class of compresent qualities ordinarily named "Socrates"—and it would be too bad for our program of translation if the only defining property we could think of were the property of being a quality of Socrates!

A similar translation-schema is, of course, the phenomenalist theory of the self suggested by Hume. It may well be that in predicating a mental state of a mind, this mental state is said to

belong to a certain class or sequence of mental states. But to say this, illuminating as it may be, is a far cry from stating a rule for translating such sentences as "I am thinking of dinner now" into class-membership assertions mentioning definite classes of mental states without the use of personal pronouns.

Now, if the regularity theory is formulated as a schema of translation of causal statements, it may turn out to be invulnerable by the criticisms that threaten it if it is put in the form of a rule of translation. Suppose we say: "*c* caused *e*" is equivalent to the conjunction of "*c* immediately preceded *e*" with *some law* of the form "every instance of C is followed by an instance of E." Then we have merely indicated the sort of thing that is being said by singular causal statements without providing a fixed recipe for carrying out the translation into the language of correlations. It is easy to confuse here two different statements:

(1) "It was the antecedent *c* which caused *e*" is equivalent to "there are classes C and E containing *c* and *e* respectively, such that every member of C is followed by a member of E."

(2) "It was the antecedent *c* which caused *e*" is equivalent to some law of the form "every member of C (a class containing *c*) is followed by some member of E (a class containing *e*)."

(1) is a rule of translation which is fatally struck by the sort of criticisms already stated; but the same criticisms are irrelevant to (2), which is no more than a schema of translation. We cannot, according to (2), tell what classes of events are implicitly referred to by the singular judgment "*c* caused *e*" by simply looking at the descriptions used for identifying the cause and the effect, and therefore cannot lay down a fixed and general rule of translation for this class of statements.[4] Take, for instance, the causal judgment "the living room window was broken by the tennis ball which John threw at it." It is easily conceivable that the event described as "John throwing a tennis-ball at the living-room window" is unique in the sense that there is no other instance of this kind of event. In that case the regular sequence implicitly referred to could hardly be defined in terms of the class "John throwing a tennis-ball at the living-room window," which is a unit-class.

But how are we to tell just which classes are implicitly referred to? On the one hand, the classes must not be too small, for other-

[4] In this respect singular causal judgments are analogous to predications of probability upon singular events, like "it is probable that it will rain here tomorrow," which are commonly held to refer implicitly to some "reference-class" or other. Notice that the frequency theory of probability can function as a fixed, general rule of translation only as applied to *complete* probability statements, statements explicitly mentioning a reference-class.

wise there is no point in speaking of "regular sequence" at all. On the other hand, it seems that if the classes are fairly large then the generalization is not sufficiently safe from counter-instances to warrant the claim that it is implicit in the singular judgment which we assert with such confidence. Yet, this argument overlooks that we tacitly provide our generalizations with the protective clause "*ceteris paribus.*" The effect of this clause is to make strict counter-instances impossible and to turn the appearance of an exception into a stimulus to modification of the antecedent. To illustrate in terms of the above example: we may well feel as confident of the truth of the generalization "whenever a massive object is thrown with force at a window, then, *other things being equal,* the window breaks" as of the truth of the singular causal judgment about John's action, even though we know of the existence of unbreakable windows that can take such impacts without damage; for by the clause "other things being equal" we just announce our intention of modifying rather than abandoning the generalization when faced with unusual circumstances that disrupt the regularity of sequence. It might be replied that insertion of that protective clause is, after all, only a way of characterizing the generalization as a probability-implication; and that since singular causal judgments clearly *claim* certainty, the regularity theory is inadequate even if formulated as a mere schema of translation. But is there really a convincing argument against the view that in saying "c caused e" we implicitly predict that *under similar circumstances* an event of the kind of c (the expression "of the kind of" being admittedly vague) is invariably followed by an event of the kind of e? If not, then we must conclude that the confidence with which we assert a singular causal judgment derives from the same protective clause which makes us confident of the truth of a corresponding generalization.

Owing to the vaguenes of the expressions "event of the same kind as c" and "similar circumstances," all singular causal judgments are, indeed, beset with ineradicable vagueness. It would, therefore, be an irrelevant criticism of the regularity theory to complain that it fails to give a precise meaning to such judgments. On the other hand, the advocates of the regularity theory could do much to avert such irrelevant arguments by making it clear that they never meant the regularity theory as an analysis of causation in the sense of a fixed and general rule of translation.

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SOME REFLECTIONS ON THE MEANINGS OF FREEDOM

PHILOSOPHERS, and others, have used the word *freedom* to refer to two different loci in human experience so consistently and for so long that it can be fairly said that there are two basic meanings of freedom. It is the purpose of this paper to specify these meanings and to suggest certain unhappy consequences for social theory and social action which ensue when the distinction is ignored or forgotten.

Of the two basic meanings of freedom, one is quite easy to explain. We are free when we can think or do what we choose to think or do, and we are unfree in so far as we are prevented from so thinking or doing. Freedom here means absence of obstructions to the execution of decisions. I shall call this meaning of freedom "freedom of action." It should be noted that freedom in this sense includes freedom of thought in so far as thought is accounted a kind of action which can be obstructed. A distinction is perhaps in order here. I do not include indoctrination among the impairments of freedom of thought in the sense under discussion. The mind shaped by propaganda is not for this reason obstructed in thinking as it chooses—the limitation is rather on what it is able to choose. It is possible, however, so to intimidate people that they will avoid even the entertainment of certain ideas accounted dangerous. This is what is intended by the phrase "thought control" and this would be an impairment of freedom of thought in the sense in which it is a freedom of action. There are various species of freedom of action and these are familiar under the name of civil liberties. Although the achievement and maintenance of freedom of action raises many controversial problems, its meaning is quite clear and non-controversial. No one denies that freedom in this sense is possible nor that there are various degrees of it.

The second meaning of freedom, I shall call "freedom of decision." It has been covered with confusion throughout the history of critical thought, and controversy has centered more on the question of its existence than on how to achieve and maintain it. It has to do with the making of choices and conditions antecedent to them, whereas freedom of action has to do with conditions subsequent to them. It intends to name the absence of compulsion, coercion, causation, determination, or necessitation of our choices. Freedom of decision thus refers to a situation in experience which is prior to the situation referred to by freedom of action. The centuries of controversy over freedom of the will have been about this prior situation. The theories which have resulted range from Hobbes' flat denial that there is any freedom possible other than freedom of

action to M. Sartre's attribution to man of an order of creativity which has been traditionally reserved to God.

Mr. Dewey's insistence on the continuity of decision and action has focused attention on the important truth that action without decision is blind and decision without the possibility of action is fruitless. With this no one could quarrel. But coupled with the desire of the contemporary naturalist temper of philosophy to steer wide of any hypostatizations of the human power of choice, it has, I fear, led to the neglect of the phenomenon which lies at the bottom of free will controversies and which we very much need to have recalled to our attention. Decision and action may be stages in a single process, but it will become evident that what has to be done to liberate decision is different from, though surely related to, what has to be done to liberate action; and if, by neglecting the distinction, we neglect to do one or another of these things, the result is almost sure to be loss of as much of both liberties as we may presently enjoy, and certainly not the gain in both that we urgently need.

I would like at this point to suggest an approach to a specification of the meaning of freedom of decision which neither makes it trivial nor requires the taking of sides in any controversy over the freedom of the will. Freedom of decision properly names a common human ability the existence of which can be made apparent to anybody since it enters to some degree into everyone's experience. A complete definition and description of this ability would be a major undertaking, but it is necessary here only to call attention to a minimal condition of it. Persons who wish to defend some theory of freedom of the will will want to mean more by freedom of decision than this minimum, but they will surely accept this much. Persons who wish to defend determinism will still admit the existence of the phenomenon I seek to describe.

It is apparent on the least reflection that only a very small number of the countless motions and actions in which our bodies are involved can reasonably be counted as voluntary, and few of them are deemed capable of being brought under our control. We can do little about the character of the motions within our bodies. The actions of the organism as a whole are to a large extent directed by reflexes and largely unconsciously formed habits. Add to this the massive influence on our behavior of subconscious influences which we have only recently begun to understand, and only a very small segment of behavior is left to include in the class of voluntary behavior, that is, of conduct.

Voluntary behavior has been variously defined for various pur-

poses. For purposes of ethics, there appears to be substantial agreement that in all cases of voluntary behavior the incentives to behavior and actual behavior are mediated by conscious choices, have been so mediated in previous similar situations, or are deemed capable of being or having been so mediated. The first class includes behavior which follows upon a conscious choice; the second includes behavior which follows upon a habit or habits in the establishment of which conscious choice played an important part; the third includes the cases where we believe that conscious choice was possible although it was not actual. Although the third class raises special problems, it does not invalidate the contention that the mediation of conscious choice is the defining characteristic of the voluntary.

I propose to employ the word *belief* to name any conscious thought entertained by an agent as a candidate for acceptance or rejection. A conscious choice terminates in the acceptance of a belief. In this wide meaning of the term, beliefs may be held tentatively or with great assurance, and they may be true or false, good or bad, or otherwise valuable or disvaluable.

Beliefs arise from many sources and the question of influences on belief-formation is very complex. It will be sufficient for the present discussion to point out that perhaps the two greatest classes of influences on belief formation could be called authority and direct experience. The brief time span of our lives, geographical limitations, the limits of the scope of attention, and many other factors so limit our direct experience that it cannot be held to be the source of as many of our beliefs as we sometimes suppose. The great bulk of our beliefs we doubtless accept from other persons. But whether they may be traced to an origin in our immediate experience or to some authoritative agency, our beliefs may be either true or false, and one cannot determine the tenability of a belief by tracing it to its source. Authorities are fallible; and beliefs based on experience are interpretations of it, not photographs, and, therefore, liable to inductive and other fallacies of interpretation. When we seek to find out whether a belief is true or false or otherwise valuable or disvaluable, we may need to consider its source and appraise the reliability of that source, but the type of inquiry which is appropriate to the justification or rejection of a belief is quite different from the type of inquiry appropriate to the discovery of its origin.

We do not know the truth value, probability value, or other value of a belief unless we have appraised it in the light of some standard or standards of acceptability, or tested it by some method

of verification. Various standards of acceptability and methods of verification have been defended. It is irrelevant to the present argument what standard or method we may employ. Here, I only wish to point out that unless we have gone about the business of verifying our beliefs in accordance with some standard or method which seems acceptable to us, *we* don't know whether these beliefs are true or otherwise valuable, or not. Now comes the central point of the argument. Unless we have engaged to some degree in the type of inquiry which is appropriate to the justification or rejection of the beliefs on which our conscious choices turn, we cannot be said to have decided freely. Free decisions are thus a subclass of conscious choices, and I should also argue that free behavior, being that which follows upon free decisions, is a subclass of voluntary behavior. Where decision is not free, it is determined indirectly by the belief-forming agencies which implanted the controlling beliefs in us, however long ago this may have taken place.

Even the most ardent libertarian will admit that some of our decisions, and of course any actions to which they lead, are compelled, coerced, caused, determined, or necessitated by passions or other drives within or by environmental conditions without. It is emphasized here that even among our non-coerced decisions many are indirect responses to belief-forming agencies. It is only when the beliefs upon which our decisions turn have been examined by us and accepted as warranting our adherence to them, or are in some way known by us to follow from beliefs which have been examined, that the decisions in which these beliefs operate can be said to be free, and the actions, if any, which follow from them can be said to be wholly our own doing. To put the root of the matter simply: What I call my beliefs are wholly mine only if I have earned them by inquiry into their truth or other value.

The more censorious among us might account the entertainment and advocacy of ideas held to be wicked or subversive to be the major scandal of the human intellect. I should rather think it to be the paucity or even the absence of examined ideas in the minds of persons held to be educated and intelligent. Wicked or subversive ideas which are held for reasons and can be questioned are far less dangerous than the inhuman mechanical advocacy of ideas which are held for no reason at all. What we call our beliefs, and become vociferous or hostile about if they are questioned, are frequently not ours, because we haven't the vaguest notion whether they are true, false, good, or bad, because we simply haven't thought about the matter. Many of the beliefs we profess—and the beliefs of philosophers are not always exceptions—are merely impressed

in us by belief-forming agencies. Indeed, I suspect that there may be a positive correlation between the emotional intensity with which a controversial belief is defended and the absence of reflective thought about that belief by him who thus defends it.

The thesis is here proposed that freedom of decision involves, as a minimum, examination of the beliefs on the basis of which we decide. It is not contended that all examined beliefs are true or otherwise valuable, nor is it argued that after examining beliefs all people will agree. The point insisted upon is simply that without inquiry into the truth or other value of our beliefs, *we* are not in a position to know whether they are acceptable or not. This may or may not be all there is to freedom of decision. I am not concerned with that very important question. My only point is that my decision is surely not a free one unless it turns on a belief held by me to be true or good and for some reason. Notice that the question as to how much examination is necessary to make a belief really our own is not raised, nor is the method of investigation or the standard, or standards, of acceptability specified. These are important matters, of course, but because of what was called the major scandal of the human intellect, it is not necessary here to go into them. Since so many people entertain and defend beliefs on the basis of no examination whatever, we need demand only that there have been *some* examination by *some* method or standard if the decisions which turn on them are to be called free at all.

It may be objected that the spatial and temporal limitations of our experience render the examination of all the beliefs we require to guide our actions and attitudes practically impossible. There is a sense in which this is true. Yet we can make up our minds on the basis of the best evidence we can muster as to the truth or falsity, goodness or badness, of certain key beliefs which underlie, and are presupposed by, many of our lesser beliefs. We can, that is, establish a more or less systematic core of examined beliefs which will give our attitudes and actions some direction and some consistency. These will of course be modified as evidence and experience accumulate, but, at any time, our conduct will be grounded in beliefs which are truly ours. Freedom of decision is, therefore, something that can be, to some extent at least, achieved.

In a society in which propaganda techniques are well understood and belief-forming agencies are organized and expert, the importance of persons capable of freedom of decision should be obvious. So far as I am aware, only liberal education can develop this ability; it could surely not be produced by legislation. On the other

hand, civil liberties need legislation to instate and safeguard them as well as education that they may be recognized and defended. What must be done to further freedom of decision is thus different from what must be done to further freedom of action.

The relation between the two kinds of freedom is likewise obvious. Without freedom of decision, the minds of men can be shaped to the pleasure of those who have the power. We have seen this occur all too frequently in recent history. When such control is achieved, civil liberties soon disappear. On the other hand, unless the freedoms of action are jealously guarded, freedom of decision becomes a mockery because wholly ineffective. When the distinction between the two meanings of freedom is ignored or forgotten, it seems inevitable that support for one or the other of them will be lacking. When this occurs, both will tend to disappear.

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COMMENTS AND CRITICISM

MR. WILD'S ONTOLOGY AND ETHICS

Mr. Wild's recent paper[1] on ontology and ethics raises again an old question and gives an old answer. The answer is not the worse, of course, for being old; but it utterly fails, as far as I can see, to answer the question posed. The question is the relation between value and existence. Mr. Wild does not wish to see them either identified or disjoined. They are to be related through the concept of "tendency." "Value is what ought to be," he says in one paragraph, and in the next, "value is the fulfilment of existential tendency."

In the course of his analysis he makes, I think, some telling points against any ontology that would omit the category of tendency or potency. I do not wish to quarrel here. I am not acquainted with any naturalist who omits altogether the sort of thing Mr. Wild wants, since it usually appears in its subjective guise of *desire;* but if there are any, they should be corrected. If we have nothing in our categorial scheme but eternal essences and instantaneous states of existence, then indeed we do not have enough to describe either movement or life. I should agree, therefore, that potency or tendency or disposition is needed.

[1] John Wild, "Tendency, The Ontological Ground of Ethics," this JOURNAL, Vol. XLIX, No. 14, July 3, 1952, pp. 461–475.

But the question I would like to raise is this: does the introduction of such a term resolve in any fashion the apparent antinomy between value and existence? Let us first clarify the problem. Value and existence are regarded as antinomic only when we wish to assert that something desired is evil, or, in Mr. Wild's terms, when something towards which we have a tendency is evil and ought not to be realized. But if "value is the fulfilment of existential tendency," then it becomes an analytic phase of a certain type of existence, namely, tendential existence, and any criticism of such values becomes absurd. If everything toward which we tended became *ipso facto* a value, then value itself becomes fact, a future fact in the career of tendency. Such a point of view, however, prevents us by definition from saying that there is something toward which we have a tendency but which nevertheless is evil. The only sense of "evil" now is frustration, the unhappiness of a tendency interrupted in mid-flight. And not only am I forbidden by such a doctrine from morally criticizing any end for which I have a tendency, I have also a *positive obligation* to pursue it. For, if the end is possible, and I do not feel an obligation to pursue it, "either my analysis of the tendency is wrong, or I do not understand myself."

If we substitute "desire" for "tendency," this view differs hardly at all from the customary naturalist view which can be found, for example, in Santayana. The good is the object of desire, and evil is what frustrates this desire. Mr. Wild is perhaps more explicit in his emphasis that tendency is an ultimate ontological category; but when we come to ethics, the view is the same.

This view, I suggest, is inadequate precisely because it fails to do justice to those "phenomena of ethics" to which Mr. Wild appeals. Let us consider a possible example: someone strikes me in anger, and instantly I find myself provided with the tendency to strike back. The fulfilment of such a tendency would be to flatten my opponent. Now, if I were a good man, I might pause an instant and reflect upon whether such a fulfilment were a value or not. I would agree that it would be a delicious pleasure; but the thought might intrude that this fulfilment of tendency was really quite evil. What basis could Mr. Wild's theory provide for such a judgment? Values for him are reducible to tendencies; hence comparison of values must be reducible to comparisons of tendencies. Tendencies, however, are factual affairs, and how are these competing facts to be compared save in terms of strength? We should have to say, therefore, that that tendency *ought* to win out which was strongest, most massive, represented a harmony of the

greatest number of tendencies, or some such other quantitative criterion. In the moral question of judging tendencies or desires, we can, according to Mr. Wild's schema, only employ factual, existential criteria, and with this our view collapses back to the view that whatever is, is right, a view which is not a moral one and which Mr. Wild elsewhere in the article rightly shuns. The situation perhaps might be saved if the values themselves had an order or hierarchy *independent* of the strengths of our tendencies or desires; but in such a case we could not define them in terms of those tendencies. To decide between a conflict of values *morally* is to recognize the independence of value from existence, whether that existence is tendential or not.[2]

Remorse is a related phenomenon in ethics. I feel remorse when I do something which I recognize I ought not to do. I *do* it, and therefore the stronger tendency was for it; but I still feel remorse, wishing that that tendency were not the stronger. Of the two tendencies, remorse was factually weaker, but ethically higher. And in sacrifice I decide on ethical grounds to give up the whole mass of my existing tendencies in favor of some value of compelling worth. How could the theory of tendencies account for this?

In short, the problem concerning the relation of value to existence arises whenever we wish to *criticize* tendencies. Such a criticism must be made either from the point of view of other tendencies or from the point of view of values independent of tendency. Mr. Wild's theory can make such a criticism only from the point of view of other tendencies; the claim of any particular value is validated in terms of some existential state of affairs: strength, power, massiveness, or number of tendencies. Mr. Wild's ethics thus reduces itself to facts, facts about tendencies in comparison with one another, or with a possible human character. Such a theory offers, however, no light on the moral question, which asks *which* tendencies, *which* character, *which* harmony of desires.

I am in complete agreement with Mr. Wild when he wishes a more profound ontological analysis for ethical problems. But I do not think that any ontology which is itself a categorization of facts, no matter how subtle the categories, can be adequate to the moral problem which requires a criticism of fact. And fact remains fact even though it has tendencies.

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[2] Cf. Eliseo Vivas, *The Moral Life and the Ethical Life* (University of Chicago Press, Chicago, 1950).

BOOK REVIEWS

Men and Movements in American Philosophy. JOSEPH L. BLAU. New York: Prentice-Hall, Inc., 1952. xi, 403 pp. (Prentice-Hall Philosophy Series.) $4.50.

This is a textbook history by an able and experienced teacher who knows the primary sources at first hand and has worked directly from them. That he commands the secondary sources too (as we should expect from his having prepared the valuable "Guides to the Literature" for Professor Schneider's *History of American Philosophy*) is also sufficiently evident, though his notes make little reference to them.

After a short prelude on "The Puritan Background," there are nine substantial chapters extending from "Colonial Materialism and Immaterialism" to "The Emergence of Naturalism." In each chapter, a general survey of the period or movement, giving due attention to minor figures, is followed by separate accounts of three major thinkers, except that the first chapter so honors only two: Jonathan Edwards and Cadwallader Colden. In the last chapter, for instance, the first section, "The Naturalistic Temper," reviews naturalistic tendencies in American thought from the beginning and traces the development of a naturalistic movement in recent decades; and this is followed by sections entitled "Poetic Naturalism: George Santayana," "Rationalistic Naturalism: Morris R. Cohen," and "Experimental Naturalism: John Dewey," in an order of increasing sympathy. In spite of their criticisms of each other, Cohen's and Dewey's forms of naturalism are presented as complementary, each requiring completion by the other. But it is Dewey, and particularly the Dewey of *A Common Faith*, that has the last word.

In effect, there are thirty-six assignments averaging slightly less than ten pages. The paragraphs are short and so, for the most part, are the sentences. There are no long quotations, but many short ones, usually of a single sentence or less. The book seems tailored to fit the single-semester or single-quarter college course. On the other hand, if supplemented by source readings, such as those in Professor Blau's *American Philosophic Addresses*, it will meet the requirements of a year's course.

What of other readers? If they can overcome their initial dismay at the reduction of twenty-six American thinkers to the metered equality of the standard-length assignment, they will find that the textbook frame has not prevented the author from writing a better history than several that are less academic in form. Like all its predecessors, it takes but inadequate account of the varied

and changing social contexts of our philosophic enterprises; but its surveys are fresh and judicious, and at least a half-dozen of its analyses of the work of major thinkers are the best in any general history, regardless of length.

There are of course blemishes, but they are remediable without abandonment of the general plan. There are, in the first place, minor inaccuracies in statements of fact. For example:

> John Dewey, in a sketch of the development of pragmatism in America, suggested that when William James referred to pragmatism as "a new name for an old way of thinking," the historic ancestry implied was to the work of Francis Bacon. . . . Whether the specific reference was to Bacon, we do not know. [P. 230.]

James did not so refer to pragmatism; it was Addison Moore and not Dewey who misquoted him as doing so; Dewey did not make this suggestion; and we know that James's specific reference was not to Bacon.

There are also opacities and confusions of interpretation, as in this passage on the role of reaction-habits in Peirce's theory of meaning:

> When a habit of this sort has been developed it is possible to say that our reaction to that stimulus has become generalized, or, to put it another way, we do not react to the particular event but to its probable consequences. The "idea" of the stimulus, or its "meaning," is its consequence in our behavior. These ideas, these meanings, are universals which are not merely names for a common quality abstracted from things, nor entities existing in some non-natural realm. They do exist, objectively, "among things." They are habits in nature. Nature can and does form habits, but not as freely as organic being. [P. 244.]

Most readers will be hard put to construe the sentence, "It is only as there is a transcendental and universal self underlying the individual that [Emerson's] self-reliance can be translated as individualism" (p. 124); or to guess in what ways the argument for James's will to believe "parallels" that for his theory of emotion (p. 258); or in what sense for Peirce "physical laws are habits of lesser inclusiveness than the laws of vital organization" (p. 242); or what effect is meant in the statement that "The effect may differ from individual to individual, yet the method should lead all to the same conclusion" (p. 250).

And will not the author be shocked when his students hand back to him in examination such assertions as that "it is likely that Peirce's acquaintance with the works of Scotus was quite perfunctory" (p. 244); that in James's pluralism "the world is composed of an infinite diversity of qualitatively different stuffs" (p. 259); that Santayana's Realm of Truth is that "particular part of the Realm of Essence which is composed of those essences which

do occur in existence, which exist somewhere" (p. 332); and that "Matter is the realm of the biologically primary substance or stuff which is the unknowable yet ever-present basis of action" (p. 331)?

But if these are blemishes, as I think they are, they can easily be removed in a second printing, and they do not greatly detract from the usefulness of the first. On the whole the book is accurate, incisive, expertly adapted to its purpose, and, within its self-imposed limits, adequate. It is also well produced physically. The only ground of complaint in that regard is that chapter titles are used for running heads on both facing pages. It would have been better to use chapter titles only on the left, and section titles on the right.

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Philosophie de la valeur. RAYMOND RUYER. Paris: Librairie Armand Colin, 1952. 216 pp. (Collection Armand Colin, N° 277.) 260 fr.

There is need for a work in the field of theory of value which shall be summary and circumambient, which shall take up and distill the results of the many partisan researches and systematizations of the last fifty or sixty years and present an impartial conspectus of the results achieved. No satisfactory work of this nature exists in English nor probably in any other language. Laird's *The Idea of Value* is lacking in depth and is, if one may so put it, too idiosyncratic. The treatises in German of A. Stern, O. Kraus, and J. Hessen have a greater measure of profundity but are in too large measure partial and, to Anglo-Saxon eyes at least, provincial. Although the present work, by Professeur Raymond Ruyer of the Université de Nancy, comes closer in some ways than any of the others to filling the need for an epitome of axiology, it nevertheless fails in certain essential respects. Unfortunately, therefore, much as one would wish to be able to say to one's colleagues and students, "Now here is the book which fairly synthesizes the field for you," one cannot, and the need remains.

Philosophie de la valeur is divided into two parts, preceded by an historically oriented Introduction and followed by a summary Conclusion. The first part is titled, "Description de la valeur," the second part, "Les théories de la valeur." In the first two chapters of Part I value is respectively treated of from each of the two polar aspects which, according to M. Ruyer, are implicit in the conception: namely, value as essence and as form, and value as the act of an agent. On the basis of this classifica-

tion, the third chapter of Part I discusses the relationships, hierarchical and otherwise, of given values to one another, including economic values and, of course, the classical trinity of Truth, Goodness, and Beauty.

If Part I, and especially this last chapter, is the weakest part of the book, Part II is the strongest. Here is given a series of summarizations of the various main theories of value which have been or are advocated. The statement by M. Ruyer of each position is concise, precise, and notably fair-minded. Each one is followed by a short critique which, if not always convincing, is yet interesting and stimulating. Value theories are classified in the five chapters of this part as: naturalistic theories in general (e.g., hedonism and utilitarianism, Spinoza, Freud, Marxism, Ostwald, Köhler); naturalistic theories of the agent (e.g., Hutcheson, Hume, Butler, Schopenhauer, Nietzsche, Meinong in his first phase, von Ehrenfels, Perry, Dewey—these being called psychological theories; Durkheim, Simmel, Veblen *et al.*—sociological); nonnaturalistic theories of the agent (e.g., Kant, Natorp, Cassirer, Brunschvicg, existentialism); realistic theories (e.g., Moore, Alexander, Whitehead, Windelband [called Windenband on p. 194 for the only noted misprint], Scheler, Hartmann); and theories of value as "active participation" (e.g., Meinong in his second phase, W. Stern, La Senne, Lavelle, and, presumably, the author). A brief bibliography follows. As is unfortunately usual in French treatises, there is no index.

The style is throughout characterized by the traditional Gallic clarity—of meaning, if not always of philosophical significance. No prospective reader need anticipate the sort of bafflement which faces him in attempting in their original tongues such exponents of philosophy as Plotinus, Kant, or Whitehead. There is a complete and most refreshing absence of jargon or indeed of obfuscation of any kind. The text is interlarded with numerous apt and ingenious examples, many of them not without humor. The authorities discussed, cited, or referred to cover a surprisingly large range and constitute a complete absolution of M. Ruyer from any taint of that provincialism which has been (and indeed still is) too common on both sides of the Atlantic. Among the American scholars who find a place in this work are: Gardner Murphy, R. S. Woodworth, Ruth Benedict, S. C. Pepper, Whitehead, Lawrence J. Henderson, G. H. Sabine, J. B. Watson, E. C. Tolman, P. A. Sorokin, W. MacDougall, J. S. Plant, Abram Kardiner, Margaret Mead, Thorstein Veblen, William James, W. M. Urban, Lewis Mumford, Max Wertheimer, C. L. Stevenson, E. S. Brightman, and Santayana. Perhaps the chief axiological absentee is

C. I. Lewis. The list of British scholars with whom M. Ruyer shows acquaintance would be as great or greater.

The faults which so largely frustrate the "description de la valeur" in Part I are the consequence of the general fact that the book is primarily discursive, not analytical. This occasions a great number of propositions being enunciated which leave the reader—at least the unpredisposed reader—without conviction, because he has been offered no analysis which would have assured him of the meanings of the key terms in those propositions. Thus, for example, we are told (p. 10) that an act of preference does not entail any judgment or feeling of value. Maybe it does not, but then again maybe it does. Stoicism certainly holds to the affirmative. But how are we to decide without an analysis of what this "preference" is? This we are not given. On page 13 it appears that values "n'existent pas à la manière des êtres *actuels*, dont on peut dater l'origine et la fin. Elles sont plutôt en dehors du temps, à la manière des essences. . . ." This is an extraordinary statement and should come, not in the first pages of the book, but only after a long preparatory analysis. Placed as it is, with the reader left to understand the terms in some commonsense fashion, it must appear false, if not downright meaningless. Or again (p. 30), "Le champ axiologique est polarisé selon un *optimum* indéfinissable quantitativement." But even if the reader is willing to credit the first eight words of this proposition, why should he assent to the highly controversial option elected in the last two? Because of some sort of analytical demonstration. Yes, but none is given—only appeals to examples and an elusive use of metaphors. A great point is made throughout (e.g., p. 52, p. 147) of contrasting value judgments with judgments of fact. Examples of the alleged distinction are given but no argument to justify it, i.e., an analysis exhibiting the meaning of "value," "fact," "value judgment," "judgment of fact," followed by an ostensive demonstration that their denotata *are* irreducibly different.

Similarly, on page 53 we learn that a statement of preference is not psychologically a judgment of value (Stoicism again to the contrary) unless it is implied that others ought ("devraient') to share the preference. But, aside from other difficulties, what is this key conception of "devrait"? To be sure, pages 57–60 ostensibly concern themselves with the "sentiment du 'devrait,'" but after they have been read, the reader is left with no analytic answer. About all we know is that the "devrait" manifests itself by some kind of "attirance." No doubt. So life teaches us. But what have we, other than a vague metaphor?

The pretensions of behaviorism to furnishing an adequate axiological framework are dismissed (pp. 62–63) in short order. But the dismissal is accomplished merely by the pronouncement that organic behavior has not the pattern "stimulus—organisme—réponse"; on the contrary, its pattern is "organisme—but, à travers stimuli et obstacles." This clearly does not suffice. Nor, for the purposes of rational persuasion, does the following *ipse dixit* statement (p. 65): ". . . l'activité axiologique est inconcevable sans conscience ou subjectivité." Or the next sentence in the book: "Toute finalité est axiologique." Why so? In both cases responsible parties have held otherwise. The latter proposition surely depends for its meaning and its truth, does it not?, on what analysis determines as to the semantic properties of the symbol "finalité."

We may add to our anthology of inadequacies a sentence from page 66: "L'activité axiologique est libre, parce qu'elle n'est pas l'effet de causes qui la poussent, mais qu'elle va vers un but situé en avant." Aside from the vexed issue of "freedom" which in this passage is raised but not clarified, what can be meant by, situated "en avant"? Here is another metaphor which, the more closely it is examined, the more misleading it appears. It but resurrects the old confusion of treating ends as forces mysteriously located in a not-yet-existent future, reaching now-wards to "pull" the organism in their direction. In the light of the analysis which naturalistic philosophy has in the last half-century successfully directed upon this point, M. Ruyer's statement invites the characterization of naïve.

Similarly, it is difficult to avoid the suggestion of naïveté in the repeated asseveration of the subjectivity of value. What response, in this philosophical day and age, is one to make when, in discussing the manner in which beauty is to be conveived as belonging to a painting, one finds this sentence (p. 79): "Le spectateur absent, il n'y a plus que des pigments chimiques sur une toile"?

There is, furthermore, throughout Part I a continual entanglement of the argument in several basic confusions, the recognition of which should be part of the stock in trade of all students of axiology. One confusion is that engendered by the uncertain or indifferent application to the noun *value* of the verbs *to be* and *to have*. On page 29 we read that forms *are* values in the process of embodiment. On page 32 it is held that the close connexion of value and form is shown by the fact that an object *has* no value saving in so far as it preserves its form. It is declared on page 35 that this planet, when inhabited by dinosaurs, *had* more

value than when it was bare and unformed. Yet on page 38 "un tableau splendide" is exampled as *being* "une valeur positive." But on page 42 we change verbs again and are referred to "un type à réaliser, qui, par lui-même, *a* une valeur" (italic added). On pages 82–83 the weathervane shifts again: "Les valeurs, comme l'a montré Meinong, *sont* des relations complexes de personnes et de choses" (italics added). (This latter, by the way, is as near to a definition of value as appears in any sentence of the book.) And so back and forth on page after page. Surely, if taken in any literal sense of the words, there are inconsistencies in these usages, and hence debilitating confusions.

But worse than this, there is throughout the suggestion of that most corrosive of all axiological confusions: confusion of *value* with *means to value*, especially *last means*. Particularly is this the case in chapter III, "Les divers ordres de valeurs." The order is rather of *means* to value—and this is a distinction with all the difference in the world. So with the alleged "conflit de valeurs" (p. 91 f.). M. Ruyer's first example, in which "valeurs s'opposent," is that of a manufacturer of automobiles having to choose in car design between the incompatible characteristics of power, speed, economy, comfort, and beauty, and having to compromise. But there is here no conflict of values. It is complete confusion (as well as inconsistent with many other passages in the work) to speak of these characteristics as *being* values. They are means to value, and their incompatibility is a "conflict" of means —which fact is as completely factual, not normative or esoterically axiological, as any other instance of incompatibility, logical or ontological. The values occasioned by those characteristics are perfectly compatible—in fact, in so far as the characteristics (means) are so, the values complement and reinforce one another —witness the happy owner speeding by in his powerful, comfortable, and beautiful "voiture."

Such considerations as these give an indication of why Part I of this treatise is inferior to Part II. But the latter is likewise not without its weaknesses. The discussion of hedonism, the first theory of value which it takes up (and, of course, summarily rejects), may serve as well as any other as an instance of such weakness. The presentation is on a completely elementary level and the objections stated to the theory are trivial; one example (p. 131) will suffice: "La théorie . . . devrait approuver un sadique, s'il avait plus de plaisir que sa victime n'a de peine." I am quite unable to see why, under a hedonistic axiology, I ought to approve of a sadist, even granting the specified conditions, considering that his conduct will probably be the occasion of far

more unpleasantness that pleasantness *to me* and to society (in whose unpleasant reactions I should as a social being inevitably share). Indeed, adhering strictly to hedonism, I ought *not* to approve, the precise and factual meaning of which "ought not" is: if I do approve of the conduct of a sadist, I am likely in the long run to suffer more as a consequence than if I disapprove. And this may be generalized to become a community speaking.

The failings of this work are almost wholly due to lack of analysis. It romps pleasantly enough in all directions through the axiological countryside—before it has mounted the horse. To universalize and draw the moral, such is just the disheartening failure of far and away the greater part of the axiological effluvium which ceaselessly pours forth. In a very real sense all the artifacts of man—all the books ever written, all the buildings ever built, all the paintings ever painted, all the words ever spoken, all the actions ever done—are part of the *materials* for an axiology. Nothing worthwhile is added by repeating or reproducing these materials or by simply flitting about among them in pleasant discourse. The materials are now sufficient and more than sufficient; likewise descriptions of the materials, as provided by philosophers, scientists, novelists, poets—and who not? Philosophers inclined to work in the field of axiology should now devote their full powers, not to merely *descriptive discourse,* however abstracted it may be, but to a rigorous *analysis* of every concept which is proposed as an instrument for the ordering, the generalization, and the understanding of these materials. Until the analysis has been made and until it has commanded substantial agreement amongst those competent to judge, not another word should be spoken with reference solely to the vagaries of the never ceasing stream of value phenomena. Not a word. It can only be trivial and another instance of fiddling while Rome burns.

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UNIVERSITY OF UTAH

NEW BOOKS AND CURRENT JOURNALS

BENTHAM, JEREMY: *Handbook of Political Fallacies.* Revised, Edited and with a Preface by Harold A. Larrabee. Baltimore: Johns Hopkins Press, 1952. xxxii, 269 p. $4.75.

BRIGGS, ARTHUR E.: *Walt Whitman, Thinker and Artist.* New York: Philosophical Library [1952]. 489 p. $4.75.

DESMONDE, WILLIAM HERBERT: *Self-Actualization: Loving and*

Strategic. Thesis (Ph. D.)—Columbia University, 1951. viii, 152 p. (Available from the author.)

Self-actualization through communication or "loving fusion" between individuals and in groups; sincerity, or integrity, as the complete giving of one's self to a loving bond, and the commitments generated thereby; the difficulties created when a sincere person interacts with other individuals who are not fully committed members of a loyal bond. Based on Mead's social behaviorism, Royce's philosophy of loyalty, and psycho-analytic theories.

PHILOSOPHISCHES JAHRBUCH. Bd. 61 (1951), Heft 3. *Albert Mitterer:* Anthropomorphe Biologien. *Paulus Lenz-Medoc:* Max Scheler in Frankreich. *Alois Dempf:* Edgar Dacqué und Joseph Görres. *Jabok Hommes:* Der Existentialismus—ein neuer Glaube. *Enrico Castelli:* Der Ausgangspunkt und die Vergegenwärtigung. *Walter Böhm:* Abstammungslehre. *Robert Martin Müller:* Ueber das Wesen der Materie.

STUDIUM GENERALE. ZEITSCHRIFT FÜR DIE EINHEIT DER WISSENSCHAFTEN IM ZUSAMMENHANG IHRER BEGRIFFSBILDUNGEN UND FORSCHUNGSMETHODEN. 4. Jahrgang, Heft 5, Mai 1951. *Hans M. Peters, Georg Scheja, Hugo Kuhn:* Probleme der produzierten Form. *Werner Wagner:* Versuch zur Kritik der Sinne. *Manfred Thiel:* Die Distanzproblematik in der Philosophie. *Hemmo Müller-Suur:* Die Ganzheitsfrage in der Psychopathologie. *C. Bennholdt-Thomsen:* Entwicklungswandlung. *Thomas Würtenberger:* Der Jugendliche vor dem Strafrichter. *Paul Wohlfarth:* Das Griechisch-Orthodoxe in der Welt Dostojewskis.—4. Jahrgang, Heft 7, Aug. 1951. *K. Lothar Wolf:* Urbildliche Betrachtung. *Wilhelm Troll:* Biomorphologie und Biosystematik als typologische Wissenschaften. *Adolf Remane:* Das Problem des Typus in der morphologischen Biologie. *Ernst Kretschmer:* Der Typus als erkenntnistheoretisches Problem. *Kurt Strunz:* Zur Methodologie der psychologischen Typenforschung. *Ernst Lewy:* Die Lehre von den Sprachtypen. *Dr. Kröplin:* Bericht der Universität Hamburg.—5. Jahrgang, Heft 1, Feb. 1952. *Konrad Lorenz:* Über tanzähnliche Bewegungsweisen bei Tieren. *Paul Th. Hoffmann:* Die Ursprünge des Bühnenwesens und die Entwicklung des religiösen Dramas und Theaters. *Irene Seydel:* Zur Hegung des mittelalterlichen Theaters. *Willi Flemming:* Das Raumproblem des Theaters. *Heinz Kindermann:* Grundformen des komischen Theaters. *Margret Dietrich:* Der Wandel des theatralischen Welt- und Menschenbildes. *K. Lothar Wolf:* Die Deutsche Akademie der Naturforscher (zum 300jährigen Bestehen).—5. Jahr-

gang, Heft 2, März 1952. *Erich Trunz:* Über das Interpretieren deutscher Dichtungen. *Ingrid Kohrs:* Ein Gedicht Georg Trakls. *Elizabeth M. Wilkinson:* Gemeinschaftsarbeit bei der Textinterpretation eines Hölderlin-Gedichtes. *Helmuth von Glasenapp:* Probleme der Übersetzung indischer Texte. *Manfred Thiel:* Das Problem der philosophischen Interpretation. *Herbert von Einem:* Fragen kunstgeschichtlicher Interpretation. *Ernst Fuchs:* Das theologische Programm Rudolf Bultmanns. *Günther W. Mühle* und *Albert Wellek:* Ausdruck, Darstellung, Gestaltung.—5. Jahrgang, Heft 3, April 1952. *Kurt Rossmann:* Ernst Hoffmann in memoriam. *Hans Blumenberg:* Philosophischer Ursprung und philosophische Kritik des Begriffs der wissenschaftlichen Methode. *Wolfgang Metzger:* Das Experiment in der Psychologie. *Hans Thomae:* Die biographische Methode in den anthropologischen Wissenschaften. *Ewald Jammers:* Die Methode der Musikwissenschaft. *Heinrich Faust:* Die Grenzen der Wettervorhersage.—5. Jahrgang, Heft 4, Mai 1952. *Hans J. Wolff:* Typen im Recht und in der Rechtswissenschaft. *Jürgen v. Kempski:* Zur Logik der Ordnungsbegriffe, besonders in den Sozialwissenschaften. *Werner Knapp:* Möglichkeit und Ziel einer Typologie in der Burgenkunde. *Theodor Schieder:* Der Typus in der Geschichtswissenschaft. *Joh's. Erich Heyde:* Typus. Ein Beitrag zur Typologik. *Friedrich Brock:* Die Bedeutung des Typusbegriffes für die biologische Eigenweltforschung.—5. Jahrgang, Heft 5, Juni 1952. *Wilfried Zeller:* Über die psychophysische Entwicklung des Kindes. *Oswald Kroh:* Allgemeinpsychologische Folgerungen aus kindespsychologischen Erkenntnissen. *Hans Heinrich Muchow:* Die Flegeljahre als Zivilisationsphänomen. Eine neue Theorie der "Vorpubertät." *Udo Undeutsch:* Somatische Akzeleration und psychische Entwicklung der Jugend der Gegenwart. *Rudolf Sieverts:* Der Jugendliche im Strafvollzug. *Helmut von Bracken:* Wandlungen der menschlichen Persönlichkeit im mittleren und höheren Alter. *Werner Wagner:* Zur Psychopathologie und Phänomenologie der Vergreisung.

NOTES AND NEWS

George Santayana, born December 16, 1863, died at Rome, September 26, 1952.

Dr. George L. Kline of Columbia University has accepted an appointment as Visiting Assistant Professor of Philosophy at the University of Chicago for the academic year 1952–1953.

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